ol 14 No 5 (New Series No 41) April 1954

ANALYSIS

UNIVERSITY OF MICHIGAN

MAY 21 1954

PERIODICAL READING ROOM

Edited by
Margaret Macdonald
with the advice of

A. J. Ayer
R. B. Braithwaite
Herbert Dingle
A. E. Duncan-Jones
C. A. Mace
A. M. MacIver
H. H. Price

CONTENTS

THE IDENTITY OF INDISCERNIBLES
D. J. O'Connor

Mr. HAMPSHIRE AND PROF. RYLE ON DISPOSITIONS
Alan R. White

SYNONYMITY, AND THE ANALYSIS OF BELIEF SENTENCES
Hilary Putnam

ON DEFINING 'SOLUBLE'—REPLY TO BERGMANN
Thomas Storer

NOTES ON BACK OF COVER

TWO SHILLINGS AND SIXPENCE NET

ASIL BLACKWELL · BROAD STREET · OXFORD

ANALYSIS 14 : 5. March, 1954

THE IDENTITY OF INDISCERNIBLES

By D. J. O'Connor

I SHALL assume in this paper that the principle of the Identity of Indiscernibles is a necessary proposition to the effect that it is impossible for two or more distinct individuals to possess all their properties in common. By an individual, I shall mean any entity that possesses qualities (including relational properties) and is not itself an instance of either a quality or a relation. (This is substantially McTaggart's definition of a substance.[1]) As I have stated it, the principle is, as we shall see, pretty vague. But it may avoid needless ambiguity if we postulate that it is a necessary principle. Leibniz, for example, does not make it quite clear whether the principle is a necessary one or not. He offers proofs that seem appropriate to a necessary statement but, on the other hand, he cites empirical evidence in its favour.

However, I wish for the present to leave open the question whether the necessary character of the Identity of Indiscernibles entails that it is merely analytic. One reason for this is that we have now to ask what we are to understand by the term "property" and its synonyms "quality", "attribute" and so on. There are some senses of these words in which the principle is manifestly true but equally manifestly trivial. If it is to be worth discussing at all, we have to construe it in a sense that is not patently tautologous. Unfortunately, there is no obvious way of guaranteeing such a non-trivial construction by a simple qualification of the statement I have given above. For example, it is very difficult, as we shall see, to find a definition of "property" that will not beg the question one way or the other before we start to discuss the principle at all. There are indeed some obvious examples of "properties" that will trivialise the Identity of Indiscernibles at once. Let us suppose, for instance, that there are two individuals, A and B, and postulate that they possess all their properties in common. That is to say, let us suppose that if any property P is a property of A, it is also a property of B. Then if we interpret the word "property" in a very wide sense, it is plain that our supposition is self-refuting. For A must have at least the property of not

1 The Nature of Existence, p. 67.

being B and conversely B has the property of not being A. But this consequence follows from the mere fact that we are talking about two distinct individuals. In other words, it is an analytic statement that if A is other than B, then it is other-than-B. Thus we must restrict our interpretation of the term " property " to exclude identity and difference if we are to make the Identity of Indiscernibles interesting and philosophically significant. We may state this restriction more generally by adopting McTaggart's rejection of any interpretation of the principle that makes its truth follow analytically from the fact that it applies only to sets of two or more individuals.

But this does not remove the difficulty. For we can still ask : what sort of properties do we have to exclude from the purview of the principle in order to make sure of this? Some of them are obvious enough ; but some of them are more elusive and others are borderline cases. And we may find that in putting these doubtful cases on one or other side of the borderline, we are not *proving* or *disproving* the maxim at all but are merely making it analytic or self-contradictory by edict. If we do this, we frustrate our purpose by depriving the Identity of Indiscernibles of any philosophical interest.

Let us start with an argument that looks plausible enough. If we list the properties belonging to a given individual A, we shall have a set of properties, P_1, P_1 . . . . P_n, which must be mutually compatible or *compossible*, to use Leibniz's word. If they were not, they clearly could not all be predicated truly of A. Now if we take these properties one by one, it seems plain that not only *may* each of them qualify more than one individual but it is essential to their nature as properties that they should be capable of doing this. There might, for example, be only one red thing in the whole universe. Nevertheless, we commonly suppose that it would still be inherent in the nature of the quality " red " that it should be *capable of* qualifying an indefinite number of individuals.

Thus we can say two things :

(i) if we suppose that every member of a given set of properties S is compatible with every other member, taken singly or collectively, we may conclude that if any individual A can have any property P_n in S, it can also have P_{n+1}.

(ii) if a property P_n can characterise any individual A, then it can also characterise any other individual B.

And from these two statements it would seem to follow recursively that if any individual A can have *any* set of compossible

properties then any other individual B can have *the same set* of properties. And this is contrary to the maxim of the Identity of Indiscernibles. For this maxim asserts that to give a complete description of an entity is to *individuate* that entity.

This will perhaps make it clear how extraordinary a claim is made by the principle under consideration. The supporters of the principle do not openly deny that any simple property must be of such a nature as to be capable of qualifying an indefinite number of individuals. But they assert nevertheless that there is always one property that is unique in every individual in the sense that the compound property consisting of *the conjunction of all the attributes* that qualify a given individual must be uniquely instantiated. The point of such a claim must either be that the totality of properties that qualify an individual has some emergent characteristic of uniqueness or that in any given totality of properties, all of which are co-instantiated, there must always be one special property of this unique kind. However, the first alternative seems to be a quite ungrounded assumption which amounts to no more than a reassertion in a disguised form of the maxim under dispute. But the second alternative has some plausibility because there do seem to be some characteristics that possess such an individuating function.

Suppose that I have on the table before me two spheres of pure silver that are identical in size, colour and every other empirical attribute (other than relational properties). I can, of course, still tell them apart because they occupy different places at the same instant and can only occupy the same place at different instants. I can identify the places that they occupy by assigning coordinates in an arbitrarily selected system of axes. For example, sphere A is at x_1, y_1, z_1 at t_1 while sphere B is at, say, x_1, y_2, z_3 at t_1. Now are we to say that "having the coordinate set (x_1, y_1, z_1, t_1)" is a *property* of A? If we do so, then clearly the principle of the Identity of Indiscernibles is true for any individuals that can be identified by their spatio-temporal characteristics. It will still remain an open question if it applies to *all* individuals, since not all individuals are obviously spatio-temporal existents.

What will be the consequence of saying that "having the coordinate set (x_1, y_1, z_1, t_1)" is a property? There is one very important and striking consequence, namely, that some properties are intrinsically incapable of being shared by more than one individual. But if this is so, why do we call them "properties" at all? Might we not more appropriately call them by some such

title as "principles of individuation"? For it is commonly taken to be a defining characteristic of a property that it is capable of qualifying an indefinite number of individuals and to be indifferent to the spatio-temporal occasions of its instantiation. Do we not need convincing reasons for extending the range of application of a term in this way? And must we not enquire carefully into the consequences of such an extension? In this position, there are various expedients open to us. If we wish to maintain the truth of the Identity of Indiscernibles, we can look for other examples of compound properties that cannot be shared by more than one entity even though the simple attributes of which such compounds are made up *are* thus multiply predicable. Or we might even look for simple properties that are capable only of unique instantiation. Thus it looks as though we cannot answer the question of the truth of the Identity of Indiscernibles until we have first answered the much wider question: what is it to be a property? Thus the justification of the Identity of Indiscernibles is really a matter of justifying linguistic categories.

Let us look at these allegedly unique spatio-temporal attributes. It is clear that they are compounds of four simpler properties, three spatial and one temporal, each of which can qualify an indefinite number of individuals. By saying that they are *compounds* of four such properties, I mean merely that an instance of such an allegedly unique property is a co-instantiation of four simpler properties. Such simpler properties can of course, be co-instantiated in more than one individual provided that they are taken in groups of less than four. It is only if we assign all four coordinates to a single object that we individuate it, or, to put it the other way round, it is only in a unique individual that we find compound properties of the type "possessing the coordinates (x_n, y_n, z_n, t_n)" instantiated. Now what sort of a statement is this? It is important how we answer this question because the plausibility of the maxim that we are considering depends largely upon examples of this kind.

If we say that it is a necessary proposition that two or more material objects cannot have the same coordinate description, then we rest the necessary truth of the Identity of Indiscernibles on the alleged necessity of the proposition embodying the definition of the phrase "one and the same material object". But to do this is to do the very thing that we have already ruled out of consideration, on the ground that it trivialises the principle. That is to say, we make the truth of the Identity

of Indiscernibles follow analytically from the definition of "one and the same material object". But might we not say that the principle was necessary, for all that, because it was entailed by a necessary proposition, namely, the proposition that expresses the truth conveyed by this definition? For it might be asserted that the definition is not merely arbitrary but is based on a necessary truth about the world. We could say this for what it is worth. But in any case it can be argued that the proposition that two physical objects cannot exist in the same place at the same time is *not* a necessary proposition but a definition that is convenient only because it is based on empirical facts.

Let us suppose a world in which certain types of material objects, say, billiard balls, did not repel each other upon impact but first merged and then separated to continue on their original paths. Looking at such a material body, we could never be sure whether it was indeed one body or two or perhaps many more.[1] Or again suppose a world in which certain sorts of material bodies had the power of dividing like paramecia and then merging again. We should never know without prolonged observation of such a set of bodies how many of them there were and even then our estimate would be merely a statistical one. In such circumstances, we should probably never ask the question "how many"? unless it was suitably qualified to "how many at this instant?" or "how many on the average over a period of an hour?" In any case, we should never, in such worlds as these, apply the allegedly necessary rules "two or more bodies cannot occupy the same place at the same time" or "a single body cannot occupy different places at the same time". Let us call these propositions P and Q for short. The wording of P and Q is unfortunate in that it suggests that we have tests for sameness and difference of times and places that we do not have for bodies. But to say, for example, "one and the same body cannot have incompatible coordinate measurements" raises the same problem. (What would be the force of 'incompatible' here?) In so far as we did admit such tests for sameness and difference of times and places, our willingness to do so would be a sign that we had shifted the locus of our problem from the nature of material bodies to the nature of space and time. The point I wish to make is this: we should never, in the imaginary worlds postulated above, come to apply rules like P and Q because the facts would not support them. (Perhaps it was something like this that J. S. Mill had vaguely in mind when he

1 Cf. Max Black, MIND, 1952, pp. 155–6.

suggested that the truths of arithmetic were merely very well founded empirical generalisations). Thus it might well be argued that the supposedly necessary propositions P and Q from which the Identity of Indiscernibles might be deduced are after all empirical, though canonised by long usage and convenience into definitions.

It was suggested by Professor Broad[1] that many people have believed in the Identity of Indiscernibles because they have also believed that there is only one spatio-temporal system in which every particular must have its own place and date. And he adds that this is not necessarily true even if it is true at all. But the supporter of the principle could very well admit Broad's contention and still hold that the principle was true. For suppose that A had the property of possessing coordinate measurements x_1, y_1, z_1, t_1 in system S_1 and B had the same coordinate measurements in system S_2. A and B would now be dissimilar in that one belonged to S_1 and the other to S_2. But if they did so, they could not strictly be compared in respect of spatio-temporal position. (This is, presumably, part of what is meant by saying that they belong to different spatio-temporal systems). Moreover, what could it mean to say that A had the *same* coordinate measurements in one system that B had in another?

It will be obvious from this very brief discussion of spatio-temporal predicates that the philosophical puzzles about space and time are another of the hidden sources of our difficulty in accepting or rejecting the Identity of Indiscernibles. My conclusions so far are very tentative and amount to the following suggestions:

(i) that the supporter of the Identity of Indiscernibles has to extend unwarrantably the denotation of words like "property", "attribute", "quality" and their synonyms by weakening their connotation in a crucial respect.

(ii) that the result of this extension is, after all, only to make the principle an analytic consequence of the allegedly necessary propositions P and Q.

(iii) that even if we waive objection (ii), it is arguable that P and Q are not necessary but merely well-grounded empirical generalisations that have hardened into definitions. In a world where objects behaved very differently, we would be prepared to abandon them. And no necessary conclusions can follow from premisses that are not themselves necessary, unless such conclusions are mere analytic trivialities.

[1] Examination of McTaggart's Philosophy, Vol. I, p. 176–7.

So far then, I have tried to suggest, though very tentatively, that the Principle of the Identity of Indiscernibles is not a necessary proposition with any philosophical importance. If it is necessary it is also trivial. And if it is more than trivial, there is good reason to believe that it is false. In this, it shows the characteristic behaviour of many metaphysical propositions. They work very often at two levels. At a superficial level, they are uninteresting and harmless because trivial. But at a deeper level, though interesting and full of consequences, they are usually also demonstrably untrue.

But there is something to be said on the other side as well. In order to bring this out, I shall conclude by summarising the basic differences between the supporters of the principle and its opponents. Its supporters want to interpret the term " property" widely enough to include relational properties. And its opponents want to exclude at least some relational properties from the denotation of the word. For once we admit them, even if we reject such obviously question-begging properties as identity and difference, we are committed to properties that by their nature can be instantiated only uniquely. Admittedly, it is in part a question of language whether or not we exclude these contentious cases. But like most linguistic questions, it can be supported on both sides by empirical considerations.

There are three main reasons for wishing to reject the claim of relational properties to rank as properties. First, many such properties can, by their nature, qualify one object only. And this settles the question at once in favour of the Identity of Indiscernibles if we can show that every individual must possess at least one such property. Secondly, relational properties (unlike relations) involve a reference to individuals that is felt to prejudice the issue in favour of the principle. " To the left of " or " larger than " are relations. But the corresponding relational properties are *not* " being to the left of x " or " being larger than x " where " x " is an unspecified variable. They are, for example, " being to the left of this tree " or " being larger than the sun ". And such properties obviously have a very strong individuating force. Lastly, it is felt that to stretch the denotation of " property " in this way is to drop from the connotation of the word the notion of a capacity for being possessed in common by several individuals. And this is felt to be an " essential " feature of the concept.

On the other hand, the supporters of the maxim also have good reasons for their semantic recommendations. In support

of their proposal to extend the meaning of the word to include relational attributes, they can point out, first, that some relational properties, at least, cannot be distinguished from ordinary qualities. Adjectives like " large ", " small ", " quick ", " loud" " useful " and so on stand for attributes that are essentially relational in character. They are elliptical ways of referring to certain relational properties of individuals. Yet it would clearly be absurd to deny them the status of properties. What other status could we accord them? Moreover, even if we take such prima facie non-relational qualities as " red ", " sweet ", " fragrant " and so on, we are at once talking of relational properties if we use these adjectives in their comparative and superlative forms. "A is sweeter than B " or " X is the most fragrant " are instances of the predication of relational attributes. (These superlatives might indeed be supposed to stand for *simple* qualities which can be instantiated uniquely, if at all.) Thus it might be alleged that the opponents of the principle of the Identity of Indiscernibles cannot solve their difficulty by excluding from the category of " property " *all* relational attributes. But if they exclude merely those that admit only unique instantiation, they are begging the question in their own favour.

Thus there seems to be no way of settling this issue decisively so long as we refuse to discuss the much wider questions that arise from it. I have tried to indicate what some of these questions are—the nature of space and time, the nature of properties and relations, the principle of individuation, the antithesis between necessary and factual propositions and several others. We can settle the truth of the principle of the Identity of Indiscernibles, considered by itself as an isolated problem, only by taking somewhat arbitrary decisions on these difficult matters. That is to say, as is common in philosophical discussions, we can clarify the situation in our immediate vicinity by sweeping the difficulties out to the periphery. But sooner or later, these peripheral problems will become central in another context and we shall then find that our piecemeal methods of philosophising have shifted our difficulties from one place to another instead of solving them. And that is, I suppose, one justification of the philosophical system-building that is at present so unfashionable.

University College of North Staffordshie

MR. HAMPSHIRE AND PROF. RYLE ON DISPOSITIONS

By ALAN R. WHITE

MR. HAMPSHIRE'S denial (ANALYSIS 14.1) that statements about dispositions, and descriptions of character, involve hypothetical or quasi-hypothetical statements raises, I think, an interesting decision problem. He lists eight overlapping criteria which a disposition statement should satisfy, and suggests that " descriptions of the causal properties of things ", which he allows could be said to involve hypotheticals, have four characteristics—three of which he numbers on pp. 7–8—which contradict four of the eight previously mentioned criteria, and that therefore it is misleading to assimilate these two types of statement as both involving hypotheticals. Mr. Hampshire does not specify who his opponents are, but he would, I think, agree that Prof. Ryle (in *The Concept of Mind*) is a well-known exponent of the thesis which is being attacked. The interesting point is that Prof. Ryle expressly admitted objections of the four kinds now put forward by Mr. Hampshire, but obviously felt that they left his thesis unweakened.

Let me briefly list the four criteria with their contradictories, giving references to *The Concept of Mind* to support my view that Prof. Ryle admits all this. I have usually added a few remarks since obviously the situation could not be as neat as a tabular form suggests.

(*a*) *Criterion* (3) " a disposition must be manifested and must show itself in actual incidents "; *Objection* (1) " a statement to the effect that some particular thing is soluble in aqua regia plainly does not carry the implication that this particular thing has ever in fact been dissolved in aqua regia "; *Ryle's admission* " unless statements like the first [i.e. episodic] were true, statements like the second could not be true " (117) and " It is clear that such statements [i.e. disposition statements] are not laws, for they mention particular things or persons " (123). However there is a difference here since Hampshire is insisting that X would not be given a ' character description ' unless X itself had been tested whereas Y would be given a ' causal property description ' even if untested. I think this is true, but it could be argued that Y is given this description in virtue of the fact that it has been tested for some properties which imply the property in question. Perhaps a similar case arises in racial or occupational

character statements, which, to the prejudiced mind, do not need the confirmation of the individual case before being asserted of it.

(*b*) *Criterion* (2) character " must not change suddenly too often " ; *Objection* (2) " a particular thing may change and change again " ; *Ryle's admission* " dispositional statements can have tenses. . . There can be short-term, long-term or termless inference tickets " (125). I do not quite understand Hampshire's remarks here in regard to ' causal properties '. If he means that e.g. chemical substances change their properties frequently or irregularly I think he is mistaken. On the other hand some people do behave in a way that leads us to call them ' unstable ', ' changeable ', ' fickle ', ' chameleon-like ', ' never uses his great gifts ', etc., which I take to be statements of character (though Hampshire might disagree, see criterion (7)).

(*c*) *Criterion* (4) " the risk of error in description of character is the risk of over-simplification ", cp. p. 8 " the incidents which may count as manifestations of human dispositions are essentially various " ; *Objection* (3) " causal properties . . . manifest themselves, if at all, in specific and definitely statable reactions " ; *Ryle's admission* " some dispositional words are highly generic or determinable, while others are highly specific or determinate " (118, cp. 43–4).

(*d*) *Criterion*. In character descriptions " One provides the *grounds* upon which such a prediction can be based, but one does not, in using the present tense, actually make a prediction " (p. 8, cp. criterion (8) and p. 10 lines 9–18). *Objection*, ' Causal property ' statements do make predictions. (p. 7) ; *Ryle's admission*, Disposition statements " are inference tickets which license us to predict . . . " (124, cp. " wide range of different actions and reactions predictable from the description of some one as ' greedy ' " 118) and he always distinguishes between a prediction and a licence to predict. It should be added that Ryle would call ' causal property ' statements also inference licences (cp. 121).

In what sense then is there an issue between Mr. Hampshire and Prof. Ryle, and how would it be solved? Hampshire's interpretation of dispositional statements is " if [as he believes] it is true that any statement about character and disposition is a summary and interpretative statement of a tendency in human behaviour and calculation, it still does not follow that such a statement entails any supposition about how the subject will, or would have, performed or calculated under certain conditions". He emphasises the summarising aspect of dispositional statements even to the extent of thinking that testimonials are such

summaries; and he distinguishes between descriptions of character as it is and predictions of what it will be. Prof. Ryle, on the other hand, says "to possess a dispositional property is not to be in a particular state, or to undergo a particular change; it is to be bound or liable to be in a particular state, or to undergo a particular change, when a particular condition is realised" (43). He believes that dispositional statements are an example of that case where "we are constantly wanting to talk about what can be relied on to happen as well as to talk about what is actually happening" (116), and, I think he would add, "what has actually happened". "On hearing that a man is vain we expect him, in the first instance, to behave in certain ways" (80).

Here there seems a distinction between Hampshire and Ryle in that the former regards dispositional statements as summaries of past behaviour, while the other takes them as inference tickets for future behaviour. But the issue is again not clear cut because while Hampshire admits (p. 5) that a dispositional statement also states what is "liable to happen", Ryle sees that we do say that a man has been vain since childhood, and he believes that we unpack this in past conditionals (85). In general, however, the one regards dispositional statements as backward-looking and the other regards them as forward-looking. Hampshire's chief fear seems to be of a confusion of a dispositional statement with the predictions based on it, and Ryle's that of an assimilation of the past evidence for a statement with the statement itself. And surely both are right. Again, while Hampshire thinks that my belief that a man will act in such and such a way is "an inference from his generosity" (p. 10), Ryle thinks it is part of what I mean by calling him generous.

What I have tried to show is that the views of Hampshire and Ryle are, in general, complementary not contradictory, and that their appearance of mutual contradiction arises from a difference of emphasis. Dispositional statements about character are both forward and backward looking, and it is wrong to deny either characteristic. However, Mr. Hampshire may have in mind some views which do really contradict his, and then of course my remarks are irrelevant.

In conclusion perhaps I could take sides a little by saying that there are three points which I think Hampshire's emphasis misses; (1) Even if dispositional statements only summarise past tendencies, they could still involve hypotheticals, e.g. past conditionals of the form 'whenever', 'if ever', etc., and subjunctive conditionals. (2) Testimonials and character judgments

do commit the maker; and when he is accused of being 'unreliable', 'misleading', 'useless', and even 'downright dishonest', it is not usually because his summaries are false. On the other hand, as Hampshire says, people do sometimes act otherwise than we had expected; but then the objects that interest scientists do too, e.g. chemical substances at low temperatures. (3) Hampshire's contrast between dispositional statements as summarising and 'causal property' statements as predicting is as misleading in regard to the latter as, I have argued, it is in regard to the former. Scientists in practice seem to consider 'causal property' statements as summaries just as much as they consider them licences to predict.

University College, Hull

SYNONYMITY, AND THE ANALYSIS OF BELIEF SENTENCES

By HILARY PUTNAM

IN the paper "Carnap's Analysis of Statements of Assertion and Belief",[1] Church has advanced some criticisms of the theory of belief sentences and indirect discourse proposed by Carnap in(1). It appears that these criticisms can be met without a modification of the theory. But certain criticisms by Benson Mates (in (6)) would seem to be more serious; these criticisms are equally forceful against the widely held view that expressions with the same sense are interchangeable in all contexts. In this paper, a revision of this principle will be suggested; and a new definition of intensional isomorphism[2] will be put forward as a basis for the reconstruction of the theory of meaning-analysis in accordance with the suggested principle.

I

Church's paper (3) is divided, conveniently for our purposes, into two parts. The criticisms in part I do not apply to Carnap's conception, since Carnap intends that, for the purposes of reconstruction, a "language" is to be thought of as having precise formation rules, designation rules, and truth-rules, or as having had these made precise; and it is to be defined by reference to its rules rather than as "the language spoken in the British Isles in 1941," or something of that kind. The criticism in part II of

Church's paper, however, is supposed to apply to Carnap's analysis even when "English" and "German" are construed as semantical systems. Let us therefore turn to this part of Church's criticism.

Church begins by considering the sentence in the system E^3:
1) Seneca said that man is a rational animal.
and its counterpart in the system G:
1′) Seneca hat gesagt dass der Mensch ein vernuenftiges Tier sei.

The analysis of 1) in the system E by the technique proposed in Carnap (1) leads to the following sentence (we follow Church's enumeration):

7) There is a sentence S_i in a semantical system S such that *a*) S_i as sentence of S is intensionally isomorphic to 'Man is a rational animal' as sentence of E, and *b*) Seneca wrote S_i as sentence of S.
and similarly, the analysis of 1′) in the system G leads to:
7′) Es gibt einen Satz S_i in einem semantischen System S, so dass *a*) S_i als Satz von S intensional isomorph zu 'Der Mensch ist ein vernuenftiges Tier' als Satz in G ist, und *b*) Seneca S_i als Satz von S geschrieben hat.

But 7) and 7′), as Church remarks, are "not intensionally isomorphic".[4]

But why should they be? Suppose someone proposes the following as an analysis of 'one' in the simplified theory of types (with "Systematic Ambiguity"):
a) $\hat{x}\ (\exists y)\ (z)\ (z \epsilon x \equiv z = y)$
and suppose further that someone else proposes instead:
b) $\hat{x}\ (\exists y)\ (y \epsilon x.\ (z)\ (z \epsilon x \supset z = y))$

These are, of course, "not intensionally isomorphic". Yet there would be no contradiction involved in regarding both analyses as correct, for they are logically equivalent. And this is the only requirement that I believe can be imposed on two correct analyses of the same concept.

It is indeed an interesting fact that the analysis of 1) in the system E leads to a result which is not intensionally isomorphic to the result of the analysis of 1′) in the system G even when the analyses are constructed on the same pattern. This is easily seen to be the result of the fact that 7) quotes a sentence of E, while 7′) quotes its translation in G, and the *names* of different intensionally isomorphic expressions are not intensionally isomorphic; in fact they are not synonymous in *any* sense. We could of course construct a sentence in the system E which would be intensionally isomorphic to 7′) (let us call it '*c*)'),

and a sentence in G which would be intensionally isomorphic to 7) (let us call it 'd)'); but I do not think we should consider it as a theoretical question: 'which is the correct analysis of 1), — 7) or 7') or c) or d)?' If one is correct, then *all* are, and it does not matter that some are intensionally isomorphic and some are not.

In closing this part of my discussion, I should like to remark that if one does wish to emend Carnap's theory so that the analysis of 1) (or of 1) and 1)') will lead to intensionally isomorphic results in the systems E and G one has only to specify that the quoted sentence should not be "Man is a rational animal", or "Der Mensch ist ein vernuenftiges Tier", but the translation of this sentence into an arbitrarily selected neutral system, say the system L, corresponding to Latin. Then in 7) the words '"Man is a rational animal," as sentence of E' become replaced by the words '"Homo est animal rationale," as sentence of L', and in 7') the words '"Der Mensch ist ein vernuenftiges Tier," als Satz von G' are replaced by '"Homo est animal rationale." als Satz von L'; and 7) and 7') are then intensionally isomorphic. But I do not think the advantages are sufficiently great to warrant this revision.

We are now in a position to consider Church's final criticism. Church points out that the result of prefixing. 'John believes that' to 7) may have a different truth value from the result of prefixing 'John glaubt dass' to 7'). But this, like the remark that 7) and 7') are "not intensionally isomorphic" is a crushing blow only if one has somehow been led to agree that 7) and 7') *ought* to be synonymous.

II

Mates remarks[5]:

"Carnap has proposed the concept of intensional isomorphism as an approximate explicatum for synonymity. It seems to me that this is the best proposal that has been made by anyone to date. However, it has, along with its merits, some rather odd consequences. For instance, let "D" and "D'" be abbreviations for two intensionally isomorphic sentences. Then the following sentences are also intensionally isomorphic:

14) Whoever believes that D, believes that D.

15) Whoever believes that D, believes that D'.

But nobody doubts that whoever believes that D believes that D. Therefore, nobody doubts that whoever believes that D believes that D'. This seems to suggest that, for any pair of

intensionally isomorphic sentences—let them be abbreviated by " D " and " D′ ",—if anybody even doubts that whoever believes that D believes that D′, then Carnap's explication is incorrect."

This argument seems extremely powerful. Suppose, for the sake of illustration, that we use ' Hellene ' as some newspapers do, as a synonym for ' Greek '. Then 'All Greeks are Greeks ', and 'All Greeks are Hellenes ', are intensionally isomorphic. Hence ' Whoever believes that all Greeks are Greeks believes that all Greeks are Greeks ' and ' Whoever believes that all Greeks are Greeks believes that all Greeks are Hellenes ' are intensionally isomorphic, and so (supposedly) synonymous. Now I do not myself doubt that ' Whoever believes that all Greeks are Greeks believes that all Greeks are Hellenes ' is true ; but it is easy to suppose that someone *does* doubt this, whereas it is quite likely that nobody doubts that whoever believes that all Greeks are Greeks believes that all Greeks are Greeks.

Accordingly,

e) Nobody doubts that whoever believes that all Greeks are Greeks believes that all Greeks are Greeks.

and

f) Nobody doubts that whoever believes that all Greeks are Greeks believes that all Greeks are Hellenes.

may quite conceivably have opposite truth value, and so *cannot* be synonymous.

Mates only suggests that this may invalidate Carnap's original proposal ; Carnap, however, takes a harsher attitude toward his own theory ; he believes that his theory in its present form cannot refute this criticism.

Mates goes on : " What is more, *any* adequate explication of synonymity will have this result, for the validity of the argument is not affected if we replace the words " intensionally isomorphic " by the word " synonymous " throughout.[6] In short, on any theory of synonymity, the synonymity of f) and e) must follow if the synonymity of ' Greek ' and ' Hellene ' is assumed. If we take this seriously, there is but one conclusion to which we can come : ' Greek ' and ' Hellene ' are not synonyms, and by the same argument, neither are any two different terms. This is a conclusion which some authors would be prepared to accept, even on other grounds." [7]

I believe, however, that the felt synonymity of such different expressions as ' snow is white ' and ' Schnee ist weiss ', or (in

the use described above) of ' Greek ' and ' Hellene ', is undeniable. To maintain this synonymity involves our denying that in fact the synonymity of f) and e) follows from the synonymity of ' Greek ' and ' Hellene '. Can this be denied ?

At first blush, it would seem that it cannot. If two expressions have the same meaning, they can be interchanged in any context. We state this formally :

g) The sense of a sentence is a function of the sense of its parts.

In a moment we shall criticize the apparent " self-evidence " of this principle. But first let us pause to make one point quite clear : whoever accepts g) must conclude that no two different expressions have the same sense, for Mates ' argument is formally sound if the interchange of expressions with the same sense is permitted in every context ; conversely, whoever believes that two different expressions *ever* have the same sense must, by the same token, reject the principle. This would seem to be entirely destructive of some present theories of meaning-analysis, which appear to involve simultaneous acceptance of g) and the synonymity of some distinct expressions.

Let us now return to g). ' The sense of a sentence is a function of the sense of its parts '. Let us ask why this seems to be self-evident. There is of course the formal similarity to the ' equals may be substituted for equals ' which we learn in high-school mathematics ; but let us put this aside as irrelevant. We may suppose that if we were to ask someone who accepts this principle why it is true, he might well reply with the rhetorical question ' Of what else could it be a function? ' And just this is the heart of the matter.

Consider, for the moment, a simpler example (a variant of the famous " paradox of analysis ") : ' Greek ' and ' Hellene ' are synonymous. But 'All Greeks are Greeks ' and 'All Greeks are Hellenes ' do not *feel* quite like synonyms. But what has changed? Did we not obtain the second sentence from the first by " putting equals for equals "? The answer is that the *logical structure* has changed. The first sentence has the form 'All F are F ', while the second has the form 'All F are G '—and these are wholly distinct (the first, in fact is L-true, while the second schema is not even L-determinate). This suggests the following revision of the principle :

(h) The sense of a sentence is a function of the sense of its parts *and of its logical structure*.[8]

I believe that a large part of the " self-evidence " of g)

arises from the fact that we do not consider any alternatives: when, in particular, we contrast g) with h), I think that we find the latter principle even more plausible than the former. It is easy to illustrate the pervasive importance of logical structure as a factor in meaning: if it is through the names occurring in it that a sentence speaks about the world, it is through its logical structure that a sentence has implication relations to other sentences, and it is upon logical stucture, or syntax, that the correctness of all our logical transformations depends.

The foregoing considerations lead us, therefore, to the following modification in the definition of intensional isomorphism:

i) Two expressions are intensionally isomorphic if they have the same logical structure, and if corresponding parts are L-equivalent.

This amounts to saying that two expressions are intensionally isomorphic if a) they are intensionally isomorphic in Carnap's sense, and b) they have the same logical structure. It is proposed that "intensional isomorphism" so defined should be the explicans for synonymity in the strongest sense (interchangeability in belief contexts and indirect discourse).[9]

III

We must now consider another possible solution to the problem posed by Mates: that of Frege. According to Frege, a sentence in an "oblique" context (i.e., a belief context, or indirect discourse) does not have its ordinary nominatum and sense; rather it names the proposition that it normally expresses, i.e., that is normally its sense, and it expresses a new sense called its "indirect" or "oblique" sense:[10]

"... there is also ... indirect discourse, of which we have seen that in it the words have their indirect (oblique) nominata which coincide with what are ordinarily their senses. In this case then the clause has as its nominatum a proposition, not a truth-value; its sense is not a proposition but it is the sense of the words 'the proposition that ...'."

This view leads to the following answer to Mates' problem: even if D and D′ ordinarily have the same sense, in 14) and 15) they have their indirect senses, and they have their normal sense as nominatum. Hence 14) and 15) do not have the same sense but only the same nominatum (truth-value); and 'nobody doubts that 14)'[11] need not even have the same truth-value as

' nobody doubts that 15) ', because in ' nobody doubts that 14) ' the whole of 14) occurs in its indirect sense.

Mates' paradox is reinstated in a milder but still extremely damaging form for this theory, however, by considering the case in which D and D′ are two expressions with the same oblique sense. In such a case e) and f) would necessarily have to have the same truth-value ; and we conclude by the same argument as before that no distinct expressions can ever have the same indirect sense.[12]

This appears to be a serious defect in Frege's theory. Frege himself certainly holds that different expressions may have the same sense in belief contexts. Thus he asserts in the passage quoted that the indirect sense of ' . . . ' is the same as the sense of the words ' the proposition that . . . ' , e.g.,

j) John believes the earth is round.

and

k) John believes the proposition that the earth is round, have the same sense. But some philosophers doubt that there are propositions, and hence that (strictly speaking) anyone ever *believes a proposition.* Such a philosopher would doubt that :

l) If anyone believes the earth is round, he believes the proposition that the earth is round.

But he certainly would not doubt that :

m) If anyone believes the earth is round, he believes the earth is round.

Therefore ' someone doubts l) ' does not have the same nominatum (truth-value) as ' someone doubts m) ', and accordingly the " indirect " sense of ' the earth is round ' is *not* the sense of the words ' the proposition that the earth is round ' —contrary to Frege's assertion. But then it becomes difficult to say just what is the indirect sense of ' the earth is round '.

Thus we see that we cannot make the slightest change in the wording of a belief sentence without altering its sense. And it can now be shown that we cannot interchange different expressions in a *reiterated* oblique context (e.g., ' George believes that John says . . . ') and hope to maintain even logical equivalence. For consider :

n) Betty said that John is a Hellene.

and

o) Betty said that John is a Greek.

Since these have different senses, the result of prefixing ' John believes that ' to n) may even have a different truth-value from the result of prefixing ' John believes that ' to o).[13]

Let us imagine a case in which John says: 'Betty said that I am a Greek.' I believe that we should regard:

p) John believes that Betty said that he is a Greek.

and

q) John believes that Betty said that he is a Hellene.

as both constituting correct descriptions of this situation. Otherwise, it would appear, we are construing p) as meaning 'John believes that Betty said 'John is Greek'"; and this would amount to taking the quotation as a *direct* quotation, not an indirect one.

In any event, the case for regarding p) and q) as equivalent seems exactly as good as the case for regarding n) and o) as equivalent. To give up the equivalence of n) and o) would of course be to give up indirect quotation altogether; but to maintain it, while denying the equivalence of p) and q) is arbitrary.

In concluding, I should like to point out some applications of the concept of synonymy presented in this paper (in i) above) to some classical problems of meaning analysis. In the first place, let us consider:

r) George asked whether the property Greek is identical with the property Hellene.

and

s) George asked whether the property Greek is identical with the property Greek.

This is clearly similar to Russell's "author of Waverley". But certain differences are relevant. The theory of descriptions will not take care of the problem posed by r) and s), for 'the property Greek' and 'the property Hellene' may well be designated by constants and not descriptions, even in *Principia Mathematica*. But in the theory presented above, no difficulty arises: 'The property Greek is identical with the property Greek' and 'The property Greek is identical with the property Hellene' are simply not synonymous (this is another instance of the "paradox of analysis"); hence r) and s) are not synonymous. The case presented by Carnap in his reply to Linsky,[14] of the sentences '5 is identical with 5' and '5 is identical with V' is disposed of in the same fashion.

Finally, let us look at Mates' sentences 14) and 15) above. If D and D' are different expressions, 14) and 15) are *never* synonymous, on our analysis. Thus we are not disturbed by the fact that 'Nobody doubts that 14)' may have a different truth value from 'Nobody doubts that 15)'.

Princeton University

BIBLIOGRAPHIC REFERENCES

(1) CARNAP, R., *Meaning and Necessity*, Univ. of Chicago Press, 1947.

(2) CARNAP, R., *Reply to Leonard Linsky* (*Philosophy of Science*, vol. 16, No. 4, 1949, pp. 347–350).

(3) CHURCH, A., *Carnap's Analysis of Statements of Assertion and Belief* (*Analysis*, Vol. 10, 1950, pp. 98–99).

(4) FREGE, G., *On Sense and Nominatum*, reprinted in *Readings in Philosophical Analysis*, edit. Feigl and Sellars, Appleton-Century-Crofts, 1949.

(5) GOODMAN, N., *On Likeness of Meaning* (*Analysis*, vol. 10, 1949, pp.)

(6) MATES, B., *Synonymity* (*Univ. of California Publications in Philosophy*, vol. 25, 1950, pp. 201–226), reprinted in *Semantics and the Philosophy of Language*, edit. Leonard Linsky, Univ. of Illinois Press, 1952.

[1] Church (3). References will henceforth be by number.

[2] This refers to the explicans proposed for the concept of synonymity in (1). It was Prof. Carnap who pointed out to me the significance of Mates' criticism; and this paper owes its existence to his stimulus and help.

[3] The letters 'E' and 'G' will denote the semantical systems corresponding to English and German.

[4] Cf. (3), p. 99.

[5] Mates (6), p. 215.

[6] (6), p. 215.

[7] Cf. Goodman (5).

[8] Two sentences are said to have the same *logical structure*, when occurrences of the same sign in one correspond to occurrences of the same sign in the other.

[9] For a distinction of stronger and weaker concepts of synonymity, see Carnap (2).

[10] Cf. (4) p. 93. The translation we follow is by Prof. Feigl.

[11] '14)', '15)', etc. are used as abbreviations here and in similar positions later.

[12] This defect in Frege's theory was pointed to me by Carnap.

[13] The difference in sense could again be directly established by using our pattern 'nobody doubts that if n) then o)'.

[14] Cf. (2).

ON DEFINING ' SOLUBLE '—REPLY TO BERGMANN

By THOMAS STORER

BERGMANN'S paper[1] commenting on my definition[2] of the predicate ' soluble ' raises several issues I should like to discuss. Essentially, Bergmann questions the idiomatic accuracy, formal possibility, and probable extensions of formal definitions of this nature. I shall discuss these three criticisms in order.

1. In my paper, I so defined the predicate ' soluble ' that if ' f_1 ' were the name of a character *none of whose instances ever have been, or ever will be, put into water*, then ' f_1 ' would be soluble since, if ' g_1 ' be the name of a character, by assumption soluble, then ' $f_1 \vee g_1$ ' is a predicate fulfilling the scope of the existential operator in my definition thus rendering ' f_1 ' as ' soluble '. According to Bergmann, this does not agree with idiomatic usage. Now how does Bergmann know this? Just what does idiomatic usage have to say about a property none of whose instances ever have been or will be tested for solubility? Shall we call it " meaningless " to speak of the " solubility " of such a property? But this would be to let our formal notion of meaning depend on material considerations . . . in this case, the trivial fact that no instance of this particular property has ever been tested. Does it not, rather, behoove the analyst to make a decision in such cases? Especially since, by assumption, his decision can run into no empirical difficulties and is introduced solely to maintain his formal ideals.

The above situation is encountered frequently when one wishes to correlate terms in a formal language with terms in colloquial English. Russell met the problem when he attempted to clarify the meaning of the statement : " The present king of France is bald". Russell chose to classify this statement as false (but not meaningless) because there is no present king of France. But does this agree with idiomatic usage? Surely there is no hair on the present king of France . . . or, for that matter, any head.

This disagreement between Bergmann and myself is funda-

[1] ANALYSIS, Vol. 12, p. 44–48. All the Bergmann quotations are taken from this article.

[2] ANALYSIS, Vol. 11, p. 134–37. The definition of ' soluble ', S(x), given in this paper is : $S(x) \equiv WD(x) .v. (EF) [F(x) . (Ey)\{F(y) . WD(y)\} . \sim (Ey)\{F(y) . \overline{WD}(y)\}]$ WD(x) and $\overline{WD}(x)$ are also defined formally in this paper to have the meaning : " x is (was, will be) put into water and does (did, will) dissolve " and " x is (was, will be) put into water and does not (did not, will not) dissolve " respectively.

mental. Bergmann is saying that the analyst convicts himself, *prima facie*, of idiomatic inaccuracy whenever he makes a decision in those cases where, by assumption, no test of inaccuracy is possible. I, on the other hand, maintain that the analyst is idiomatically accurate if his formal definition captures the meaning of a term where that meaning is a well determined one; and, in addition, renders the term completely unambiguous everywhere else.

2. Bergmann's contention that my definition is "formally impossible" is based on my own footnote[1] to the effect that it would be necessary to restrict the range of the predicate variable in the existential quantification "to 'natural' predicates; *i.e.* undefined predictates or predicates whose definitions do not involve explicit reference to an individual."

Bergmann's criticism is two-fold. First, he feels that a symbolism adequate for stating the restriction I have in mind would have to be rich enough "to contain among its particulars some that could be interpreted as the names of predicates and among its predicates... one that could be interpreted as 'undefined predicate'". And secondly, Bergmann misinterprets my use of the word 'natural' by taking it to refer to those "predicates that would "naturally" occur in "laws of nature"." Since I share Bergmann's opinion on this second point, I can clear it up most easily by dispelling his misinterpretation. If I had intended the word "natural predicate" in the sense Bergmann takes me to mean, this would, indeed, enable one to "distinguish syntactically between two kinds of sentential expression, those which, after the formalism has been interpreted, we would, if they were true, call *accidental generalities*, and those which, if they were true, we would call *laws of nature*." I agree with Bergmann that the possibility of making a *syntactical* distinction of this sort is quite remote. How, then, did I mean the term "natural predicate"?

Quite simply, I intended the phrase "natural predicate" to mean "natural relative to a given class of predicates". For, given a class of predicates, $f_1, f_1, \ldots, f_n$, there seems to be a "naturalness" to sums, products, complements, and other Boolean combinations of these which would not apply, necessarily, to the class composed of the instances of f_1 and the individual a_1. In other words, "red sugar", "white sugar", "hot sugar", "un-white sugar", and so on, are "natural predicates" relative to the class of predicates "sugar", "red",

[1] Ibid., p. 137.

"white", "hot", whereas "sugar or David Hume" is not a natural predicate.

Let me now return to the first of Bergmann's criticisms regarding the formal possibility of the restriction stated in my footnote. Here, frankly, I must confess I do not see the grounds for Bergmann's difficulty. So far as I can see, one begins a reconstruction by listing a finite class, f_1, $f_2 \ldots$, f_n, of "undefined descriptive predicates"; or else indicates, meta-linguistically, an infinite set of such undefined descriptive predicates which, however, must fall into a finite set of sub-classes, K_1, K_2, $\ldots$, K_m, mutually exclusive (for independence) yet exhaustive. In the first case, one can adopt the following definition of "undefined descriptive predicate" (UDP):

$$\mathrm{UDP}_{(f)} .=. (f = f_1) \vee (f = f_2) \vee \ldots \vee (f = f_n)$$

whereas, in the second case, the following definition will serve:

$$\mathrm{UDP}_{(f)} .=. (f \epsilon K_1) \vee (f \epsilon K_2) \vee \ldots . \vee (f \epsilon K_m)$$

Naturally, restrictions imposed upon the type of reconstruction to be made might affect the possibility of using the above definitions. In the absence of such restrictions, however, it seems safe to assume that this particular objection of Bergmann's offers no serious difficulty.

3. Bergmann doubts that impredicative definitions can be widely used for the transcription of counter-factual conditionals and states that, so far as he can see, my confidence is "entirely unsupported". My disagreement with him at this point seems mainly to be a matter of opinion to be decided by future developments. Let me indicate, however, the kind of extension I had in mind by considering the problem of defining "connection" between predicates or individuals as this word might occur in such sentences as: "red is connected with green" or "there is a necessary connection between a cause and its effect".

What do we mean when we say that two predicates are "connected"? Two definitions immediately appear as candidates. First, that there is some relation that holds between the two predicates; or, second, that there is some relation that holds between everything that exemplifies the first predicate and everything that exemplifies the second. The first definition, to be an "empirical" connection, would necessitate descriptive second level relations. The second would not need relations above the first level and might appeal to elementaristically minded philosophers for this reason. Formally, the two definitions would be:

First : $C_{(f,g)} .=. (ER)\, R_{(f,g)}$

Second : $C_{(f,g)} .=. (ER)\,(x)\,(y)\,[(f_{(x)} \cdot g_{(y)}) \supset R(x,y)]$

It would also seem that one might say that x and y were connected necessarily if they instanced two connected predicates. In symbols :

$$NC_{(x,y)} .=. (Ef)\,(Eg)\,[f_{(x)} \cdot g_{(y)} \cdot C_{(f,g)}]$$

Another obvious definition would be to define the set of all f-g connections, S(f, g), which, depending on our choice of definition for C(f, g) would have one of the following forms :

First : $S(f,g) .=. \hat{R}\ R(f,g)$

Second : $S(f,g) .=. \hat{R}(x)\,(y)\,[(f_{(x)} \cdot g_{(y)}) \supset R_{(x,y)}]$

Using either of the above two definitions, we may now define " the definite f–g connection ", D(f, g), as :

$$D(f,g) .=. \pi_R\,(R \subset S\,(f,g))$$

D (f, g) may be thought of as that exhaustive relationship which *f* and *g* always bear to one another by virtue of the " f-ness of *f* and the g-ness of *g* ". It has some correspondence, perhaps, with the metaphysical notion of " real connection ". If we let PR be " prior to " and CST be " contiguous in space and time ", we may render Hume's classical definition of " cause ", in the sentence " *f* is the cause of *g* " (Cs (f, g)) as follows :

$$Cs\,(f,g) .=. (PR \subset D(f,g)).\ (CST \subset D\,(f,g))$$

Let me repeat, however, that I do not wish to assert that these definitions are " the definitions " of the idiomatic words I have attached to them. To me, they seem to be reasonably accurate transcriptions of words I have found much used throughout an ambiguous literature, to the ambiguity of which, perhaps, they contributed a share. More intricate " connection relations " may be defined, obviously, by considering the inner relations of the D(f, g) ; entailment relations, perhaps, among the S(f, g) set ; or some further D-connections among the elements of that set. If so, then these further relationships may be clarified in terms of the definitions I have just given.

University of Nebraska.